XVI. *Reply to Mr.* Cavendiſh's *Anſwer.*

By Richard Kirwan, *Eſq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. CAVENDISH's anſwer, as I conſider the greater part of mine to him as ſtill unanſwered.

In the firſt place, he ſays, that in Mr. LASSONE's experiment the effervеſcence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was diſengaged. But this could not have happened; for, firſt, the zinc, inſtead of being further acted on by the acid, was precipitated according to Mr. LASSONE's own account (p. 8.); and, ſecondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervеſcence aroſe.

2dly, With regard to the calcination of lead; though in England the ſmoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. NOSE, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and ſmoke? Mr. MACQUER even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. MONNET made minium by melting lead in a cuppel, in ſuch

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ſuch a manner that it was impoſſible it could come in contact with the leaſt particle of flame or ſmoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expreſſes his ſurpriſe at my aſſerting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the ſame as that out of which he had extracted fixed air; but, I think, I have aſſigned very ſufficient reaſons for my opinion: how far I was right will beſt appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

"I certainly imagined the two black powders you write "about to be of the ſame nature, and therefore did not at- "tempt to extract any air from the latter; but immediately "on the receipt of your favour of yeſterday, I diſſolved an "ounce of lead in mercury, and expelling it by agitation, "put the black powder, which weighed near 12 ounces, into "a coated glaſs retort; then applying heat, I got from it about "20 ounce meaſures of very pure fixed air, not $\frac{1}{30}$th of which "remained unabſorbed by water."

Fourthly, it is impoſſible to attribute the fixed air, produced by the diſtillation of red precipitate and filings of iron, to the decompoſition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he uſed, ſuppoſing the whole of the plumbago to to conſiſt of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain meaſures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100 grains of bar iron contain, according to Mr. BERGMAN, at moſt, two-tenths of a grain of plumbago; and conſequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELE, contains but one-third of its weight of fixed air; ſo that here, ſuppoſing the plumbago to be decompoſed, we can have at moſt but ſeven-tenths of a grain of fixed air, or little more than one cubic inch. If we ſuppoſe the filings to be from ſteel, 1000 grains of ſteel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the ſtrongeſt ſuppoſition, and the moſt favourable to Mr. CAVENDISH. What ſhall we then ſay, if we conſider that theſe filings were mixed with copper or braſs which contain no plumbago? and, above all, that plumbago cannot be ſuppoſed decompoſable by red precipitate, ſince even the nitrous acid cannot decompoſe it?

5thly, With regard to the power which nitrous ſelenite has of abſorbing fixed air, I muſt allow the experiments of Mr. CAVENDISH to be juſt and agreeable to my own; but it only follows, that when fixed air is in its *naſcent* ſtate, it is more abſorbable. Thus many metallic calces take it from alkalies in its *naſcent* ſtate, though in other circumſtances they will take none.

Laſtly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elaſtic in cold; beſides, I have often made the mixture without producing any ſuch durable vapour; and this will always happen, when the nitrous air is made from nitrous acid ſufficiently diluted.

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